

(21.)
ON THE

CHANGES IN THE CONSTITUTION

OF

FEVERS AND INFLAMMATIONS

IN EDINBURGH,

DURING THE LAST FORTY YEARS.

BY

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THE CHANGES

IN THE

CONSTITUTION OF FEVERS AND INFLAMMATIONS.

FOR some time past, to those whose professional life embraces a period of thirty years or upwards, and to others whose curiosity has led them to look thus far back upon the experience of their predecessors, it has appeared a remarkable fact, that blood-letting, a principal remedy formerly in almost all fevers and acute inflammations, has been gradually abandoned in a great measure for the treatment of these diseases in Edinburgh, and throughout the country at large.

So radical a revolution in medical practice required explanation: and two have been offered. According to some, the change in practice has been owing to an improved acquaintance with the pathology of diseases, rendering it apparent that bleeding never was the proper remedy for fevers and inflammations. According to others, a change has arisen by degrees in the constitution of such diseases, rendering the loss of blood, though formerly useful, an inadmissible treatment in the present day.

Among the supporters of the former view, the most conspicuous hitherto has been my colleague Dr Bennett. The only advocate of the latter, who has yet appeared in print, is Dr Alison.

It is not every one who is in a condition to speak to this question with the authority of a personal observer. For it is chiefly among those who have had long experience in hospital practice, that the requisite direct knowledge is to be found. In this city there is now, besides Dr Alison, no practitioner but myself, whose hospital experience extends continuously so far back as we must go for the facts. It is, therefore, in present circumstances, almost a debt, due by me to the history of medicine in our country, to record what I have been able to observe illustrative of the question that has arisen among us.

But the cause of the abandonment of blood-letting as a remedy, is far from being a new or recent subject of consideration with me. The views I have been led to entertain, and which I propose to lay systematically before the Medico-Chirurgical Society on this occasion, are not at all the offspring of the present controversy. They

were come to in part three and twenty years ago, were adopted in their entirety soon afterwards, and owe to controversy, whether past or present, nothing except the time and mode in which they are now promulgated; for they have often been categorically stated, though in disjointed fragments, in my lectures on *Materia Medica*, as well as in those of *Clinical Medicine*, since so early a date as 1835. This will be shown presently.

It is, I think, about eight years ago, that, conversing with Dr Alison about the abandonment of bleeding as a familiar remedy by both of us, as well as by our professional brethren generally, I found he had been led by his own experience to entertain the same ideas with myself, as to an important change having taken place in what may be called the constitutional part of many febrile and inflammatory diseases. I therefore took the liberty of urging upon my colleague, that, as he and I were the only hospital physicians of sufficient standing and opportunities of continuous observation, to be able to speak to the point in dispute from direct personal knowledge, it was a duty on his part to commit the results of his experience to print; and I promised that I should one day follow his example. It may be in some measure owing to this exhortation that he first took some notice of the subject in a clinical lecture in May 1850 (*Monthly Journal of Medical Science*, xi. 157), and again in a clinical summary, published in the following January (*Ibid.* xii. 71). To these sources, and to his more recent express dissertation, entitled, "Reflections on the Results of Experience as to the Symptoms of Internal Inflammation, and the Effects of Blood-letting during the last Forty Years" (*Edinburgh Medical Journal*, March 1856), I refer for an able and dispassionate statement of numerous facts, which cannot be denied; and cautious deductions which it will be difficult to refute. These papers enable me to avoid many details on the same topics, and to produce my own observations in a different shape. For I propose to state the results of my experience in the form of a disquisition "On the Changes which have taken place in the Constitution of Fevers and Acute Inflammations in Edinburgh during the last forty years."

I wish that, in the following remarks, it were practicable to avoid entirely the controversial element. I hope to do so in general. But it is impossible to escape observing at the outset, that in the very ingenious inquiry and eloquent exposition brought before the Medico-Chirurgical Society, on January 21st, by Dr Bennett, on the question whether the disease *Pneumonia* has undergone any change in its character to account for the abandonment of bleeding as its remedy in the present day,—he made two omissions, to his own advantage in the controversy, but not to the illumination of the matter in dispute.

In the first place, he confined himself in a great measure to the local portion of the disease which he investigated, overlooking the

fact, that it has a constitutional ingredient also. Fevers and acute inflammations alike consist of local derangement and constitutional disturbance,—the local affection in inflammations, and the constitutional one in fevers, being primary. The essence of the local inflammation in pneumonia, no doubt, as Dr Bennett forcibly urged, may be quite the same now as it was when Dr Gregory practised, and even when Hippocrates flourished. But it does not follow that the constitutional accompaniment,—consisting of a disturbed circulation and a disordered nervous system,—has likewise been always the same. Nor did Dr Bennett advance a single argument to prove it to be so,—though in this proof lies the whole essence of the matter. For, in the treatment of pneumonia, for example, the constitutional element of the disease cannot be discarded, merely because it happens to be secondary to the local morbid action. On the contrary, it is often the main object of regard in the treatment; because on the one hand, it may be such as to aggravate local mal-action, or on the other, to forbid remedies otherwise available for its removal.

Secondly, Dr Bennett also omitted, or made little mention of, other acute inflammations besides pneumonia, and altogether overlooked idiopathic fevers; in all of which the same change of treatment, the relinquishment of bleeding, has equally occurred, as in the instance of pneumonia, which alone he took into consideration. It cannot surely, however, be represented, that in point of fact the change of treatment in pneumonia led to the same change in treating all other acute inflammations, and still less all fevers. I can bear witness, at all events, that the abandonment of bleeding in idiopathic fevers preceded by a good many years in this city its abandonment in acute inflammations; and that its gradual abandonment in the latter took place simultaneously in all acute inflammations; and not, as Dr Bennett urges as to pneumonia, because of an improved diagnosis; for there are several internal inflammations whose diagnosis did not make any sensible progress either immediately before, or during, the change in their treatment.

In truth, the disappearance of bleeding from the method of cure for fevers, though now almost forgotten, is a much more remarkable fact than the surrender of it for the cure of local acute inflammations. And I believe we shall not be far from the truth, if the latter event be ascribed mainly to the former; and simply, because, the attention of practitioners having been first drawn to the change occurring in the constitution of fever, they were thence led naturally to observe an analogous change going on in the constitution of the symptomatic fever attending acute inflammation.

At any rate, the question,—Whether a constitutional change has taken place in fevers,—stands on its own basis, is altogether as important as the question of a constitutional change in inflammations, and may be considered quite apart. It has, I think, important

bearings on the latter question. But it ought to be inquired into independently, and on its own merits. And therefore, first of all, I shall treat of—

I. *The constitutional changes which have taken place in the Fevers of Edinburgh during the last forty years.*

This may seem a long period for me to include, since I propose to confine myself entirely to personal observation and its fruits. But the fact is, that while only a medical student of two years' standing, I became virtually a practitioner, by commencing the duties of Resident Medical Officer in our Infirmary, in the autumn of 1817. That period was critical in two important respects. For in 1817, Continued Fever first began to show itself in Edinburgh as an extensive epidemic, or, at least, first attracted attention as such; and within a few months afterwards blood-letting, which had been brought into vogue by the prelections and example of Dr Gregory, then Professor of the Practice of Physic in our University, attained its highest reputation and widest range in the cure of diseases at large. Perhaps I may have had even some share in propagating it. Certainly no one practised it with more energy or confidence; and this I acknowledge partly because it is of moment with reference to what appear to me the sound principles of practice in the present time.

For two years I enjoyed ample opportunities of observing the features of disease, while holding the position of Medical Clerk or Resident Physician in our Royal Infirmary, or in a fever hospital, which it became necessary to add for relief of the ordinary Infirmary wards from the increasing epidemic. After this I lost sight of the fevers of Edinburgh for a period of eighteen months, while absent in London and Paris. On my return I was for a few years more an observer only of the practice of others in our hospital. In 1827, however, I was again a responsible observer, having been appointed Ordinary Physician in that year; and this chanced exactly at the outbreak of a second wide-spread epidemic of fever, requiring the temporary establishment of a fever hospital of 150 beds, of which I had sometimes sole charge. This office I resigned in 1832, when my translation to the chair of Materia Medica, with a share in the duties of Clinical Professor, gave me an opportunity, which I have annually made use of ever since, except during the first session, for extending my acquaintance with the diseases of the working-classes of the community.

These facts are stated merely in order that it may be seen what opportunities have been at my command for direct and continuous observation during a long term of years. No one, as it appears to me, can possess such adequate opportunities for the inquiry, which it is my desire now to elucidate, as one who has had long and continuous hospital experience,—no physician, at least; for in early life his means of observation in private practice are necessarily much too limited.

It will be seen from the accompanying Table, derived from the Annual Reports of our Infirmary, that in the year 1817, continued fever spread in Edinburgh to an extent wholly unexampled before, so far as can be shown by any available records. The causes of this calamity need not be inquired into now. But it was traced, with much likelihood, to the united agency of a succession of previous defective crops; a want of employment, and consequent poverty, among the labouring classes; and the commencement, on a great scale, of that migration of the poorest class of the Irish, which has streamed more or less ever since into the great towns of Scotland, overstocking them with labourers, condensing their population, and planting among the native citizens habits of overcrowding, uncleanness, and improvidence.

TABLE SHOWING THE ANNUAL NUMBER OF FEVER CASES IN THE ROYAL INFIRMARY SINCE THE BEGINNING OF THE CENTURY.

12 Mons. to Dec. 1800, 329	12 Mons. to Dec. 1819, 1088	12 Mons. to Oct 1838, 2244
" " 1801, 161	" " 1820, 638	" " 1839, 1235
" " 1802, 156	" " 1821, 327	" " 1840, 782
" " 1803, 232	" " 1822, 355	" " 1841, 1372
" " 1804, 323	" " 1823, 102	" " 1842, 842
" " 1805, 175	" " 1824, 177	" " 1843, 2080
" " 1806, 95	" " 1825, 341	" " 1844, 3339
" " 1807, 110	9 Mons. to Oct. 1826, 450	" " 1845, 683
" " 1808, 111	12 Mons. to Oct. 1827, 1875	" " 1846, 693
" " 1809, 186	" " 1828, 2013	" " 1847, 3688
" " 1810, 143	" " 1829, 771	" " 1848, 4693
" " 1811, 96	" " 1830, 346	" " 1849, 726
" " 1812, 103	" " 1831, 758	" " 1850, 520
" " 1813, 75	" " 1832, 1394	" " 1851, 959
" " 1814, 87	" " 1833, 878	" " 1852, 691
" " 1815, 96	" " 1834, 690	" " 1853, 574
" " 1816, 105	" " 1835, 826	" " 1854, 168
" " 1817, 485	" " 1836, 652	" " 1855, 201
" " 1818, 1546	" " 1837, 1224	" " 1856, 180

The nature of that epidemic is a point of much greater immediate interest. For to its peculiarities, I apprehend, may be traced the unreserved adoption of blood-letting as a principal remedy for many years afterwards in most fevers and inflammations.

The epidemic fever of 1817–1820, presented two well-defined forms. One was that which it has been for some fifteen years the fashion to call *Relapsing fever*, on account of its extreme tendency to return after a perfect intermission of several days. Its proper nosographical name, however, is Inflammatory fever,—the *Causus* of older authors, or *Synocha* of Cullen and his contemporaries,—by which last convenient name we all knew it when it first appeared, but which writers of the present day seem to have a dread of using, probably through fear of being thought to adopt Cullen's pathology along with his nomenclature.

This fever, which is described in detail in a paper supplied by me on the subject of continued fever, to Dr Tweedie's "Library of

Medicine," in 1840, was characterised by its very abrupt invasion, often rendering the patient prostrate within an hour; by the continuous and urgent suffering from febrile anxiety, restlessness, burning heat, rending headache, and irritability of the organs of sense, throughout the whole period of perfected fever;—by its abrupt departure, often in two hours, with free sweating, between the fourth and seventh days, most generally on the fifth;—by a complete apyretic intermission succeeding, so that in a few days the patient might be out of bed, or even walk out of doors, or sometimes actually travel to some distance without difficulty;—by an abrupt relapse, ushered in with severe rigors, taking place on the fourteenth day from the beginning of the primary attack, and not to be averted by any precautionary management;—and finally, by a second abrupt sweating crisis on the third day of relapse, leaving the patient greatly prostrate, and with a slow convalescence to pass through, but without any vestige of fever after the expiry of the few brief hours of critical sweating.

I must omit here, for brevity's sake, all minor characters, and all varieties, of which there were several of great interest,—confining myself to the prime features recognisable in a great proportion of the whole cases. These features, as now sketched, are so peculiar and so prominent, that one would imagine it impossible not to recognise such a fever. And, nevertheless, when it reappeared, after an absence of fourteen or fifteen years from our city, it was not at first recognised, though my account of it in the "Library of Medicine" had been published only two years before. It was accordingly christened with a new name, *Relapsing Fever*; and an author had advanced far in printing a book about it, before he discovered that he was not a new observer in nosology.

This fever, or *Synocha*, as it appeared in the epidemic form in 1817–20, was eminently of the sthenic, phlogistic, or inflammatory type. Essentially it was a primary fever, without local inflammation. The pulse ranged from 120 to 160; it might be large, or it might be small; but if the latter, it was wiry, if the former, cordy, that is, always hard and incompressible; and no contrast could be greater than the sudden descent of this accelerated, irresistible pulse, to the soft fluent pulse of 72 or less, which marked the influence of the resolving critical sweat. Then, the heat of the body ranged from 102 to 105, at times even to 107; the patient's sense of dry burning heat was intense, so that he would lie, and still suffer from heat, with only a linen sheet over him, and the window open, in such cool weather as, for example, we have now in the month of March; and the feeling of heat, imparted to another person touching the skin, was decidedly great and often pungent. Another remarkable fact was the florid hue of the venous blood, and its extraordinary impetus in escaping from the open vein. The blood indeed sometimes appeared as if it spouted from an artery. "Aliquando sanguinein e venâ ita floridum et per saltum fluentem

vidi, ut mihi metum arteriæ subjacentis vulneratæ primo perculerit." [*Thesis Auctoris*, etc., 1819, p. 29]. In conformity with this state of things, the skin presented a vivid glow, not on the countenance merely, but likewise over most of the body; the thirst was excessive, the sense of pulsation in the head and chest distracting; and, as the senses were acute, the suffering was extreme.

I am able to speak with some confidence to all these facts; for, besides observing them on numberless occasions in others, I experienced them myself no less than three times during fifteen months of this first epidemic. Two other peculiarities may also be mentioned. There was not often much delirium, except in children, who, it is well known, are apt to rave in all febrile diseases. And there was little tendency to local secondary inflammation in those not exposed to cold and other privations at the period of invasion or recently before; but again, a great proneness to such complication in the class of society constantly liable to the predisposing and exciting causes of local disease.

I have here described a disease which many now present have never seen; because, according to another remarkable law in its constitution, it never occurs, at least I have never seen it, except in the epidemic form. But, from my description, no one ought to have any difficulty in recognising and acknowledging it as a very peculiar form of essential, idiopathic, or primary continued fever, characterised, among other particulars, by a truly sthenic, inflammatory, or excited state of the circulation. We shall see presently what became of it. But in the first place, let me describe the rest of the epidemic of 1817-20.

Nothing in the shape of fever can well be more unlike this form of it, than what is known to all in the present day by the name of typhus. In the ordinary run of cases of typhus, the invasion is slow and gradual. There is nothing like vigorous reaction, or sthenic fever, at any period of its course; the pulse being easily compressible, the heat little elevated, the skin dingy, seldom florid, the jactitation trivial, the senses benumbed, the mind clouded or disturbed by delirium. Then the fever runs on unabated for at least eleven days, more generally for fourteen or seventeen, or even more. There is seldom any tendency to crisis by sweating; the rule of critical days is very often violated; the fever never ceases abruptly, but, as it approached insidiously, so does it pass off by little and little. The liability to relapse is very slight. And when death results, which is a frequent event, a state of congestion of almost every important organ of the body precedes death for many days, and occasions delirium, coma, and many other deplorable symptoms, which it would be inconsistent with the necessary brevity of the present sketch to describe. Lastly, this form of fever is not confined to epidemic seasons, but may be met with at all times. Hence it is, possibly, that the name *typhus* is rooted in medical language, and has even passed into common speech; while the

name *synocha* is unknown in the latter, and is too little known even to professional men.

Now, this form of fever was comparatively little seen during the epidemic fever of 1817–20. A true unmistakeable typical typhus, as all physicians have understood it in this country since the days of Cullen, could scarcely be said to form part of that epidemic. I doubt whether there was more of it than its ordinary sporadic proportion, or even so much. This is no “*pro re natâ*” proposition, got up for the occasion,—now for the first time,—the offspring of controversy. Fortunately I can satisfy every one of the contrary by testimony of the time,—by my own testimony in my Inaugural Dissertation, presented to the medical faculty of this University in March 1819, and consequently in the very middle of the epidemic. “*Febris continua Edinburgi formam typhodem, raro assumere dicitur: et in hâc peste equidem rarissime aliquem signis typhi a Culleno notatis laborantem vidi*” [p. 9]. I well remember, indeed, the interest excited among the students of our hospital by the admission of a case of characteristic typhus into the wards.

Instead of a true typhus, the epidemic was principally made up of a different form, which would be classed as typhus in the present day—and perhaps correctly—but which, at the period in question, when external characters, or grouping of symptoms, were fully as much considered in nosological arrangements as the pathological essence, or anatomical characters of disease—was called *synochus*. *Synochus*, in the Cullenian nosology, is an essential fever, beginning as *synocha*, or inflammatory fever, and ending as typhus. However much it may be the fashion with anatomical nosologists to under-rate Cullen’s system of classification, and to throw aside his descriptions and definitions of disease, I must say, that I even now do not know a better descriptive definition than his of the fever which constituted the most frequent form of the epidemic of 1817–20. For, at the beginning of the attack, no one could distinguish this variety from the inflammatory [or relapsing] fever already described. There was the same abrupt invasion, the same vehement pulse, the same high temperature, the same intense restlessness. In a word, for six or seven days there was no sign to show that the attack was not to end, like the inflammatory fever, in an abrupt sweating crisis. But, instead of that, the pulse and heat, at the close of the first week, abated somewhat in force; the acute, anxious inquietude passed by degrees into prostrate torpor; the mental faculties became clouded, and either delirium or stupor, or both, stole on imperceptibly; till at length, in ten or eleven days, the features of typhus were clearly unfolded.

Still, however, traces of the primary sthenic fever remained. Some cases ended by a slow, gentle diaphoresis, beginning on the eleventh, and going on till the fourteenth day—a termination, by the way, which I have seen about half-a-dozen times in the true typhus, both mild and grave, which has prevailed sporadically during

the last three or four years. Other cases seemed to yield slowly about the eleventh or fourteenth day to blood-letting, without any critical sweat or other evacuation. Occasionally too a crisis by spontaneous hemorrhage appeared to justify the treatment by bleeding, and illustrated the never-failing descriptions of ancient authors. It is also worthy of mention, that the termination of fever by hemorrhage, has not been witnessed for many years in Edinburgh, either in epidemic periods or during the non-epidemic intervals. Another very important fact was, that in every mode of progress—in fatal cases equally as in recoveries, the pulse was generally observed to retain very long a considerable measure of that fundamental volume and force, which formed a predominating character of the early stage of the disease, as well as of the whole course of the inflammatory fever or synocha. One consequence of this state of the circulation was, that, in the secondary typhous stage, blood-letting, even practised from a vein, was borne well in general; and another consequence, not less remarkable and undoubted, was, that wine was not sustained with the ease and certainty which might have been expected from the proofs of typhoid prostration actually present. In my thesis wine, as a remedy, is thus adverted to:—“*Stadio synochi icterodes progressu, typhoque puro, sed varietati præterea vix ulli, beneficio fuit.*”

It will not seem unnatural, that at this period, and long afterwards, the two most prevalent forms of the epidemic—the one which terminated generally on or before the seventh day by sweating, and the other, which, instead of doing so, passed into a subsequent stage of typhus—were regarded as one and the same disease. Were we wrong? There was a time when, impressed by new phenomena in the progress of our epidemics, I was inclined to conclude, and have actually admitted in my lectures on clinical medicine, that we had been in error; that synocha, or relapsing fever, is a separate disease, “*sui generis*,” that true typhus is another, and that our synochus, or intermediate form, is only a modification of typhus. But, on later re-consideration, I am much inclined to revert to the original doctrine, so far as to see in synochus an ally of synocha, rather than of typhus, and possibly nothing else than an unresolved synocha. We have an analogue in malignant cholera. As observed for many years in the East Indies, if it did not prove fatal in the stage of diarrhœa, spasm, and collapse, cholera came to speedy resolution, without any remarkable consecutive phenomena. But, after it reached Western Europe, the patient had generally to pass through a secondary stage, much resembling profound typhus, and dependent apparently on suppression of urine. Nevertheless, the two forms are the same disease, however different in their course; and no man has hitherto thought of making two diseases of them. Synocha and synochus, as they occurred in 1817-20, were not more different in their characters and progress. And yet, if they be assumed to be one and the same disease, what is to be thought of the relations of typhus,

into which, undoubtedly, a characteristic synochus may be easily traced by the imperceptibly varying shades observed in a long succession of cases, even in one epidemic, but still more in several epidemics—the inflammatory introduction being, so to speak, softened by little and little, in a series of cases, till at length it is suppressed and lost altogether.

Thus, by contemplating the many varying forms and shades of fever in one or more epidemics, we are irresistibly led to the great question—Are all these forms of continued fever essentially different, or essentially the same, in origin and essence? As to this question, I will only say here, that, instead of becoming clearer as our information about fever extends, it has hitherto seemed to me to be rendered, on the contrary, more dubious than formerly; and that, above all, it is not to be solved in the negative, with the facility and confidence which have been brought to the inquiry by some purely anatomical physicians of the present day.

But I must not allow myself to be diverted by this theoretical episode from the main action of my subject.

I think that, if what I have said of the great features of the remarkable epidemic fever of 1817-20 be dispassionately viewed, no one can feel surprise that bleeding became for it a much-favoured remedy. When the epidemic broke out, two other modes of treatment were in vogue—the diaphoretic plan and the cold affusion. Diaphoretics, though utterly useless, were generally given then, as they are given still, for the sake of doing something. We certainly did no good with them in 1817-20. The cold affusion, too, a potent agent, and indicated by the great increase of animal heat, proved equally useless. It gave relief for half an hour, but was of no permanent advantage, though often repeated; and, after myself experiencing the agonising central headache, substituted instantly by it for the prior and more bearable general headache of the fever itself, I felt no desire to inflict such intense suffering upon any fellow-creature. But a fever, with such vivid reaction, demanded some sedative. Blood-letting, the most powerful and certain of all, was resorted to. And such were the good effects apparently obtained with it, that it soon came into universal credit, and was carried to what will very naturally now seem extravagant lengths.

The following are extracts from my own account, in 1819, of these proceedings:—"Febrin adultum implicantem raro uncis sedecim, frequentius viginti, viginti-quatuor, viginti-octo, trigintave missis expulsum vidi; et aliquando e viro procero, forti, et toroso, tres libras, vel amplius insigni cum beneficio eductæ sunt."—"Aliquando venam iterum iterumque feriri opus fuit."—"Aegroti, quos solos tractavi, raro tertiæ venæsectionis indiguerunt; alii autem in compluribus exemplis ter quaterve cum exitu optato uti sunt."—[*Diss. Inaug.*, pp. 48, 49.]

This was vigorous practice; and it was honestly and impartially enforced—sometimes to the terror of older, but less experienced fever

physicians. I well remember the repugnance and distress of an elderly medical friend in Ayrshire, when, on my taking a third attack there in the autumn of 1819, and persuading him, much against his will, to sanction blood-letting, I insisted on his waiting till I recovered from faintness on losing fourteen ounces of blood, and compelled him to finish the legitimate allowance of thirty ounces in all. And let it be remembered, that we did by no means slay our patients by such blood-thirstiness. On the contrary, the mortality from the whole forms of fever collectively, in that epidemic, did not exceed 1 in 22 at any period, and was reduced to 1 in 30, as the epidemic spread, and the remedy became more and more familiar. It was at the time universally believed, that bleeding had a tendency to ensure the resolution of synocha by critical sweat—to prevent in this way the passage of synocha into the secondary typhous stage—to moderate the secondary typhous phenomena—and even to relieve the evil tendencies to visceral congestion, which characterised the pure typhus occasionally encountered among the other forms of fever. I confess I am still loath to believe, with younger critics who never saw the epidemic of the time, that all these convictions were hallucinations. But, nevertheless, it must in candour be admitted, that the cure by blood-letting was resorted to by many too indiscriminately, and inflicted by others too energetically.

On consulting the Table, it will be remarked, that the next epidemic of fever occurred in 1826–29. The causes of its spread on this occasion were much the same as before. In 1825 immense failures took place in all branches of commerce and trade in the British Islands; and the exhaustion of the country from that cause, with subsequent bad crops, reduced the demand for labour, raising at the same time the price of provisions. Fever seems always to burst into an epidemic in any such conjuncture.

There is nothing further to be said of that epidemic, than that it presented precisely the same forms of fever, and the same constitution, with the prior epidemic. I had a full share in treating it, having been Ordinary Physician of the Infirmary and Fever Hospital during the whole period. It was during this second epidemic that I recognised more truthfully, the relapsing tendency of the inflammatory fever; and that, finding the relapse could not be prevented either by any precautions or by quina, and observing it always to occur on the fourteenth day, I regarded this event no longer as a true relapse, but rather as an integrant part of the fever. The form of synochus, or, if the term be thought fitter, of typhus with a primary stage of a week's vehement reaction, was again the most prevalent. And all forms were alike treated by general blood-letting.

At the end of it the form of Enteric typhus, or Dothinenteritis, first came into notice in Edinburgh, concurrently with an epidemic of dysentery, viz. in 1829. I may mention, however, that this form

of fever had occurred so early as 1817, though it was not recognised at that time; for I have lately found, in some old notes, a case of the date October 1817, which was clearly one of enteric typhus, and another which occurred in January 1819. This fever has always been rare in Edinburgh; and even in 1829 the cases of it were very few in number.

A brief lull ensued, as the Table will show. But in 1831-33, a minor epidemic presented itself. I know less of this than of others, because two attacks of fever in the early period, and in 1832-33, my first course of lectures on materia medica, withdrew me from the study of its features. In 1833-34 I began my duties as professor of clinical medicine. And now my attention was first drawn to a change apparently going on in the constitutional phenomena of our continued fever.

From 1831 onwards, for the long period of two and twenty years, it is obvious from the Table at page 7, that our fever never receded altogether from the tendency to put on the epidemic form. During that long interval, the annual number never fell, except in one year, below 700 in round numbers; and on four occasions, in 1837-39, 1841, 1843-44, and finally in 1847-48, it gathered strength, and became a formidable epidemic—the numbers increasing to twice, thrice, and on the last occasion, actually to six times the rather high average of the quiescent intervals. In the last of all, consequent on the distresses arising out of the failure of the potato crops, we had 8400 cases of fever treated in our hospital in the course of two years. From the year 1826 till 1854, there has been no want truly of opportunities of becoming familiar with the phases of continued fever.

In 1834 I became satisfied that a change had taken place in the constitutional character of our fevers. Synoehia had disappeared. Synoehus—a name which I may be allowed to use, now that my meaning in using it is clear—had also disappeared, or rather, its early stage had become much less clearly marked by the symptoms of vehement reaction, and so it was undistinguishable from a mild typhus. True typical typhus was much more common; and what did not come up to Cullen's mark of fully-formed typhus, was what physicians would now universally consider as mild typhus, with more of introductory reaction than we observe now, but much less than in the two epidemics of 1817-20 and 1826-29.

Accordingly, I doubted, and all the physicians of our hospital also doubted, whether blood-letting was applicable as a remedy to that fever. We could not bring about resolution by a sweating crisis with it. We could not lessen by it the depth of the typhoid prostration. And, worse than all this, our patients ceased to sustain free venesection, a few ounces of blood bringing on faintness, and the constitution refusing to rally afterwards.

It is very important for me to adduce evidence, that the statement now made is not the result of an afterthought of the present

day, but was come to at the time. This proof fortunately can be given from my lectures on *materia medica*. I cannot supply it so early as 1834, because I cannot fix precisely the date on which the lecture I shall quote on general blood-letting was first delivered, but it was written out, as I shall quote it, certainly in 1836, probably in 1835.

I could show, indeed, from my lecture on wine in fever, written in November 1833, that, even at that earlier date, my attention had been pointedly turned to a change going on for some time in the constitution of fevers, and rendering that remedy much more frequently necessary than in earlier epidemics. And it is evident from this circumstance, that blood-letting must also have been noticed to be inappropriate. But, for brevity's sake, I shall confine myself to my direct observations on blood-letting, written two, or perhaps three years, later.

“Much discussion has arisen among physicians as to the propriety of employing this remedy in *synochus*, some forbidding it entirely, as unsafe, or as tending at the very least to protract the disease; and others maintaining that, when employed at an early stage, it is invariably one of the best means of mitigating the violence of the primary, and thereby lessening the danger of the secondary stage. The truth is, however, that each party is in error, and has been led to form an absolute or too exclusive opinion, from having reasoned on too narrow a basis of observation—from having observed the phenomena only during the prevalence of a single epidemic constitution.

“I conceive, that in *synochus*, the employment of blood-letting, as a general remedy, is sometimes signally useful, and at other times decidedly injurious; and that the main cause of the difference, is a difference in the intrinsic constitution of different epidemics.

“When the primary stage of *synochus* is well-marked, that is, when the inflammatory state of the circulation runs high; in short, when the fever distinctly commences in the form of *synocha*, blood-letting will often prove a valuable remedy.” I then go on to describe how, in such circumstances, it may be employed, to cut the disease short, by promoting a sweating crisis—to mitigate the force of reaction, and so obtain a milder typhus in the secondary stage—and to combat incidental local inflammation. And next, noticing the opinion of some, and especially of Dr Welsh, a well-known writer on the Edinburgh Epidemic of 1817–20, that “the practice or neglect of general blood-letting is the main cause of the absence or prevalence of a typhoid character in different epidemics,” I proceed thus:—

“Had Dr Welsh survived a few years longer, he could not have failed to alter his opinion. Since the time he wrote, the dominant type of our fever has been gradually changing, the typhoid character having by degrees taken place of the inflammatory tendency

then prevalent; and this alteration has occurred in defiance of the practice of copious depletion. At present (1835 or 1836) cases of pure synocha are scarcely ever seen; cases of mild typhus are exceedingly common; and in cases of synochus, which are also frequent, the primary stage is imperfectly marked, the reaction seldom rising high, and the stage of depression coming on unusually soon.

“In this form of epidemic fever, then, if I was correct in laying down, a short while ago, the indications for blood-letting in synochus, free depletion must be seldom called for. The inflammatory fever seldom runs high enough in the early stage to require active depressing measures; diaphoretic crisis being almost unknown, we can scarcely expect to check the disease in its early stage, by inducing that mode of resolution; and, as for the third object of general blood-letting, the arresting of local inflammation, it would be absurd to resort to so active a measure if the local detraction of blood will answer the same purpose—which is actually the case.

“But this is not all. General blood-letting is not merely uncalled for in the synochus which has appeared for some time past: it is also in general positively hurtful. A close observer of nature cannot fail to remark, that, compared with former epidemics, our fever, for some years past, has been accompanied with a marked depression of the nervous system. This affection, which appears the main source of danger in most cases, and which ought therefore to be chiefly borne in mind in the treatment, generally shows itself very early in the progress of the fever, commonly towards the close of the first week, or beginning of the second, and is out of all proportion great, compared with the previous or coexistent reaction. It has invariably appeared to present itself in an aggravated form in those individuals who had been copiously bled at the commencement of the attack; and any attempt to draw blood from the general system after its formation is followed by speedy faintness, sinking of the pulse, and increase of general depression. So much, indeed, have these facts forced themselves on my notice, that, from having been at one time an uncompromising and somewhat promiscuous venesector in continued fever, I feel now inclined to avoid it on every occasion; and I have seldom been induced, by incidental violence of reaction in the early stage, to depart from this reserve, without subsequently seeing cause to repent having done so.

“To conclude, in former epidemics of fever in this city, the prevailing type of the disease was inflammatory reaction; in the later epidemics, the prevalent character has been nervous depression. And, accordingly, if free depletion was the principal remedy in the former instance, the main remedy in the latter has been wine, with other stimulants. As a general practical rule in all epidemics of continued fever, it may be assumed that blood-letting will be safe and useful as a remedy in the different stages of synochus, for the purposes specified above, provided the epidemic constitution of the

fever present frequent cases of pure synocha, and a strongly-marked stage of reaction in the early period of synochus."

The two epidemics of 1837-39, and 1841, which occurred subsequently to the first date of these observations, amply confirmed the doctrine of a progressive change going on in the constitution of the epidemic fevers of Edinburgh. The only fact, in the history of these two epidemics, sufficiently important to require distinct mention in this summary, is, that synocha, or simple inflammatory fever, which had disappeared with, or soon after, the epidemic of 1826-29, did not recur with the next two epidemic visitations. At least I never saw or heard of it; and for ten or twelve years about that period, younger practitioners used to listen with something like incredulity to my description of a fever so singular in its character and course.

In the epidemic of 1843-44, a more vehement one than any before it, synocha reappeared. My first encounter with it was one of those professional incidents which are not easily forgotten. In the commencement of the epidemic I had been for some months off duty in turn as clinical professor, when I called to see my colleague Dr Bennett, who was convalescing from an attack of it. He had suffered severely from that complication of fever in which there is an icteric hue of the integuments; but, though still confined in a great measure to bed from debility, he was well otherwise, and enjoying the genuine pleasures of a fever convalescent. When he had detailed to me his case, I told him he had sustained, to all appearance, an attack of my old acquaintance synocha, whose face I had not seen for a good many years; that he was not yet done with it; and that he would have another three-days' attack, commencing with rigor on the fourteenth day. Dr Bennett, surprised—I will not say incredulous—replied, that the relapse had no time to lose, as there was only three or four hours of the fourteenth day to run. It did, indeed, lose no time; for I must have scarcely reached home from his house, a distance of one mile, before the rigor set in with violence; and he had the old three days of fever again, terminating, as the primary attack had done, with an abrupt crisis by sweating. I do not know whether it may not occur to some to think that the relapse was brought about by the "Influence of Suggestion," concerning the remarkable operations of which my colleague has since discoursed lately with so much ability and eloquence. But hundreds of similar cases occurred afterwards, for which the blame could not be laid upon any such mesmeric agency. They became so frequent, indeed, as to attract universal attention; and they were thought to constitute a form of fever new to Edinburgh, until reference was made to my prior description.

This synocha, however, though so prevalent, by no means presented the same strong phlogistic or sthenic character as in the earlier epidemics of 1817-20, and 1826-29. The pulse was neither so

frequent nor so strong; the heat was not so pungent; the glow of the integuments was less lively and less general. In short, some influence seemed to have existed in one epidemic which did not exist in the others, and by which the force of the circulation was kept down. So, too, typhus, a very frequent form in 1843-44, showed itself in its most characteristic adynamic, or asthenic shape. And what I have called synochus presented a first stage of reaction, so feebly phlogistic, that the disease was very generally, and perhaps correctly regarded as typhus; and it was only those acquainted with the high phlogistic introductory stage of that form of fever in the earlier epidemics, who might at this time suspect the co-existence of a third form of fever, intermediate between synocha and typhus.

In the epidemic now under consideration, there never was a question raised about the revival of blood-letting as a remedy. And the reason must be too apparent to require mention.

A very short interval of imperfect repose ushered in the fearful epidemic of 1847-49, which consigned $5\frac{1}{2}$ per cent. of the whole population of Edinburgh and Leith, in little more than two years, to the care of the Infirmary physicians, raised the hospital population for some time to 700, and rendered it necessary to treat many of the patients in wooden erections, raised from time to time, and at last even in military tents, borrowed from the garrison for the purpose.

The cause of fever passing into the epidemic form on this occasion, and the probable cause of the magnitude of the epidemic, was the unprecedented hardships inflicted on the working-classes by the failures of the potato crop in several successive years. To the general influence of privation thus arising may also be probably added a change in the bodily constitution of the community, caused by a change in the kind of food; at least the existence of a morbid constitutional condition was amply proved by the concurrence, unprecedented for a long term of years, of scorbutus, in the city and neighbourhood.

In the epidemic of 1847-49, typhus was the prevailing form of fever, and typhus of the most adynamic kind, that I have hitherto seen epidemically. There could scarcely be said to be any intermediate form betwixt typhus and synocha; the prefatory stage of reaction being so feebly marked. Synocha itself, however, was extremely common, but with the same comparatively inert reaction which was observed in the previous epidemic of 1843-44. Enteric typhus also came in for a share, but, as usual in Edinburgh, a very small one.

Here, again, there was no question raised about the revival of blood-letting as a remedy. Many cases did very well with little else beyond attention to their comforts; and in the rest, the dominant remedy was wine, with other stimulants.

With the year 1849 there set in a series of favourable seasons,

producing abundant crops, and indirectly ample employment for the labouring classes. Under the blessing of Providence, we have been reaping the result in a continued immunity from fever, such as Edinburgh has never experienced since the dawn of my medical life.

In the interval our authorities have vigorously done their duty, by improving the drainage of the city, towards perpetuating the exemption we now enjoy from the pestilence. But there still remains to be attained the far more important and less easily attainable preventives—space, ventilation, and cleanliness, in the dwellings of our working population. How sad and oppressive is the feeling, that the attainment of these conditions would most positively deprive continued fever of its epidemic character ; but that the means, simple as they are, seem wholly out of reach ! There is really positive evidence that nothing more is wanting. I have often publicly stated—but the observation cannot be too often repeated—that I have attended a great number—not less, I am sure, than one hundred—of young medical friends and pupils, who had caught fever, chiefly during epidemics, by infection in hospital or dispensary practice ; and that not in one single instance was their fever communicated at home to their relatives, companions, or other attendants. The very simplest means, therefore, by stopping the spread of continued fever from the sick to the healthy, will divest the disease of its epidemic tendency. A little atmospheric air to breathe ; a little water for cleansing ; these are all the preventives necessary. But, for want of these elements—lavishly as nature supplies them—the epidemic developments of fever in Edinburgh, during the last thirty-five years, have added to the small number of inhabitants, who would otherwise have resorted to our hospital on account of this malady, 38,000 sick and 2000 deaths, and at a cost of L.110,000 to their fellow-citizens for hospital management alone.

Such is a sketch of the epidemic fevers which have ravaged Edinburgh during my medical experience. If the members of the Society have received from this sketch any material share of the ineffaceable impression made on my own mind by personal observation, they cannot fail to recognise something in our continued fevers, which is liable to change, though the fever itself changes not ; something which, with our present knowledge, we cannot, without a hypothesis, correctly define otherwise than as “a change in the constitutional element of fever.”

There has been no new fever. There has been no new exciting cause of fever. A varying intensity of cause is a pure assumption,—useless too, because inadequate as an explanation. Even the predisposing cause has been the same. In one epidemic only was there a discoverable difference in that respect ; for, in the last and worst of all, there had been a preparatory change of food as well as a deficiency of it. But the change in the constitution of our fever had begun at least twelve years sooner, and was perfected in the previous

epidemic of 1843-44, when no such predisposing complication had been at work.

What the real essence of that change may be, it is impossible as yet to tell; and as little can we say on what causes it may depend. If we look for light on the subject to the anatomical pathologist, who owns no pathological explanation except what the scalpel and the external senses can discover, we appeal to him in vain. If we turn to the modern chemical pathologist, who puts faith only in changes in the composition of the blood and other fluids, we appeal also to him in vain. If we call to our aid the toxicological pathologist, who sees only morbid poisons everywhere in fever, we still appeal in vain,—for anything more than a hypothesis. But, if a hypothesis will content us, may we not find a better by referring to the mysterious agencies exerted by external nature on the nervous system? That the nervous system plays an important, although perhaps little understood, part in governing the phenomena of fever, no one can doubt. That it may be directed and ruled in the exercise of such government only by changes in the structure of the tissues and composition of the fluids, few will maintain. When we know that the nervous energy may be powerfully influenced in asthma, neuralgia and other diseases, by atmospheric agencies of mysterious import, and of which temperature, moisture, barometric pressure, and electric state can give us no adequate account, it is no vain hypothesis, that some such unseen agency, exerted enduringly, or frequently, on the body, may so modify the governing power of the nervous system, as to engender for a time a change in the constitutional part of fevers.

It would not be difficult to advance much that is plausible in favour of a hypothesis of this kind. But hypothesis is no part of the aim of this inquiry. Its object is a theory,—a branch of the great theory of epidemics,—a pure theory or generalisation of facts,—and of facts which cannot be denied. This theory I beg once more to repeat: “In epidemic fevers, a change may take place in the constitutional part of the fever; and this change has been exemplified in Edinburgh during the last forty years, by a transition from the sthenic or phlogistic character in the first twelve years, to the asthenic or adynamic character in the twelve years which have just elapsed.”

If this change be admitted to have been proved, there is an end to all difficulty in accounting for the abandonment of blood-letting in the treatment of our fevers. In point of fact, I am able to state very positively, that the abandonment of bleeding in fever was suggested by observation of a change in the constitution of fever, and in the effects of the remedy on it, and not by any other circumstance, whether extraneous or intrinsic. It is impossible to ascribe such change of practice, as Dr Bennett has done in the instance of pneumonia, to an improved knowledge of disease. We have improved our knowledge of fever so far as to have been, for some time, well acquainted with the form of enteric typhus (dysentery),

which was unknown, or not recognised, at the commencement of our epidemics. But this is a rare form of fever in Edinburgh, scarcely belonging to its epidemics at all. And as to our only undoubted epidemic fevers, typhus and synocha, with their intermediates, we cannot be truthfully said to be better acquainted with them in 1857 than we were in 1830.

I have given, I hope, a sounder explanation; less flattering perhaps to the rising generation of physicians,—but surely more honourable to physic itself, more creditable to medical observation and experience, more consonant with the advanced state of medical philosophy. My own convictions on the subject are so strong, that I regard nothing as more likely, than that in the course of time some now present will see the day, when a reflux in the constitution of fever will present it again in its sthenic dress, and again make the lancet its remedy. And in that event it is not impossible, that, while we are now charged with giving up blood-letting, because it was discovered to have never been the proper method of cure,—we will hereafter be assailed by some new enthusiast in blood-letting, who, in imitation of Dr Welsh, and regardless of the fate of his doctrines, will accuse us, with equal justice, of having made our late fevers asthenic and typhous by blindly withholding their fittest remedy.

Since the first part of this paper was published in the *Medical and Surgical Journal*, I have fallen in with an old clinical lecture, being a retrospect, delivered in April 1836, of the cases occurring in the University clinical wards during the trimestre then concluded. As in this lecture my views are categorically stated, I beg to append them here *verbatim*. The lecture itself has been submitted to the President of the Society before which my paper was read.

After speaking of some points of treatment of continued fever, the lecture proceeds thus:—"I beg, lastly, to call your attention to the circumstance, that I have not in any case thought it right to resort to general blood-letting, either as a general or local antiphlogistic. I have avoided it, notwithstanding what Armstrong, Bateman, and others have written of its utility in congestive or typhoid¹ fever, because the experience of these authorities, and more especially of Armstrong, does not apply to the present epidemic. Bred a warm admirer and vigorous employer of the practice of blood-letting in an early epidemic, when the dominant type of fever was inflammatory, and when even the interpolated cases of typhus possessed so much of the phlogistic character as to be at times benefited by free general depletion, I have already lived long enough to see our fever alter entirely its type, so far as regards external characters, and not less so in its relations to remedies,—and to be compelled to acknowledge that a typhoid¹ form of fever may gradually arise out

¹ The word *typhoid* is here used in its original meaning, not in that which has been unfortunately borrowed of late from French authors, who apply it only to Enteric Typhus.

of the previous inflammatory epidemics, in which blood-letting, as formerly practised in these, is useless, nay, hurtful. I have not myself bled a patient in continued fever for two years past : I have hitherto seen no reason to regret having so acted ; and, on the other hand, I have seldom had a patient under my care, who had been previously bled to any material amount, without speedily witnessing as the consequence, unusual exhaustion of the nervous powers, aggravated symptoms of congestion, and irregularity and failure of the circulation. General blood-letting has not appeared to me to have any effect in diminishing congestion or nervous oppression ; and almost every local symptom it might be employed to subdue, has yielded readily enough to milder and local means."

This extract proves that a change in the type of fever must have attracted my earnest attention so early at least as 1834 ; and that the observation of the change had preceded the abandonment of blood-letting as a remedy.

II. *The constitutional changes which have taken place in the Eruptive Fevers of Edinburgh during the lasty forty years.*

In pursuing the branch of this inquiry which has been now brought to an end, much facility and certainty have been attained through the large scale of observation supplied by the hospitals of this city, and many great epidemics of continued fever. It is to be regretted that the same conditions are unattainable in regard to another common class of zymotic diseases, the *Eruptive Fevers*. One can scarcely suppose that typhus and inflammatory fevers should have undergone the important change in their constitutional element which I have endeavoured to describe, without a corresponding change having been also observable in measles, scarlatina, and small-pox.

As to small-pox, that species of eruptive fever may be set aside at once, because it has never been in my time so prevalent as to furnish a trustworthy scale of observation to any professional man in Edinburgh.

Measles and scarlatina have unluckily been too often frequent enough, and have repeatedly broken out with force in the epidemic form during the last forty years. But on account of the tender age of those who chiefly suffer from these diseases among the working classes of the city, only a very small proportion repair to our hospital for treatment. I have already admitted, that in such circumstances a physician, who did not begin his profession as a general practitioner, is apt to be unfavourably placed as an observer in early life ; and at a more advanced period he is scarcely better off, because he sees few cases of such diseases as measles and scarlatina, except those of unusual severity and danger. But, indeed, in an inquiry like the present, in which statistical considerations must constantly pervade the thoughts and influence the judgment, although they may not be capable of assuming the definite form of actual num-

bers, I must frankly say, that I have some doubt of the adequacy of the opportunities possessed by any medical practitioner who has not had the advantage of comprehensive and compact observation in a great hospital. He must be a busy general practitioner who sees forty or fifty cases in private practice during any one epidemic of scarlatina or measles. But whoever imagines that forty or fifty cases in private practice will constitute a safe basis of induction in the present inquiry, is little aware of the principles of statistical research,—of the disturbance which is apt to arise from the influence of collateral circumstances. There is one class of medical men, however, whose opportunities of study are commensurate to the object now in view. In our institutions for the maintenance and education of children in reduced circumstances, epidemics of measles and scarlatina frequently break forth with intensity. Here so large a field of observation is not so imperative as in practice in general society. For, as we have to deal throughout a long term of years with subjects always of the same age, drawn constantly from the same class of the community, and nurtured exactly all alike, it follows that many important disturbing collateral influences are excluded. If, therefore, there be any medical officer of a large institution of this kind, whose experience extends back to the date of 1817-20, when for the first time, or even to 1827-29, when for the last time, continued fever attracted attention in Edinburgh on account of the highly phlogistic or sthenic form which it presented, he will fill up a most important blank, and greatly advance the cause of truth in the present inquiry, by making public his experience as to the constitutional character of the various epidemics of scarlatina and measles which he has had occasion to treat from these dates down to the present time.

Meanwhile I may say,—but with the reserve inseparable from such an introduction,—that I cannot call to mind, in the course of my own experience, anything which does not support the general doctrine, that measles and scarlatina have passed, like continued fever, from a comparatively sthenic type to one comparatively asthenic, during the last thirty or forty years. If I may trust my recollection, that change has been less steady than in the case of continued fevers,—having been marked by ebbs and flows in its course. But, at the same time, I have a firm persuasion, that for at least twelve years after 1817 both of these eruptive fevers presented themselves to my observation in a highly phlogistic shape; while they have unquestionably tended, on the other hand, to take on the asthenic or adynamic character during at least the fifteen years now concluded.

My earliest recollection of *Measles* carries me back to a date ten years anterior to the period hitherto included in this inquiry, and when I was a mere boy. I have a vivid remembrance of an epidemic having then prevailed in Edinburgh, which, probably on account of its fatality, or the dark hue of its eruption, and a strong tendency to gangrene from pressure, was called the Black Measles

in non-professional society. Few families in the wealthier ranks of the community escaped its ravages; so that an alarm seized parents somewhat like that which spread through all ranks during the first invasion of malignant cholera. So far as I am aware, no full account of it ever appeared in print. I should not be justified, therefore, in alluding to it, were it not that I find some remarkable allusions to it in notes taken by me in 1817-18 of the Lectures of Dr James Gregory, then Professor of the Practice of Physic.

After describing measles as a disease generally presenting itself in the phlogistic form, and requiring antiphlogistic treatment, especially bleeding and purging, both for preventing pulmonary inflammation, and as the only probable resource for removing it when present, Dr Gregory referred to a "putrid" or "typhoid" form of measles as having been first described by Dr Watson of London, from the observation of two epidemics in the London Foundling Hospital in 1763 and 1768. He then mentioned that a similar epidemic had ravaged the city of Edinburgh in 1807-8. It broke out in the end of July 1807, and prevailed more or less till August of the following year. One hundred and fifty children were said to have been carried off by it in the middle ranks; and one practitioner assured Dr Gregory that he had himself lost twenty patients. The eruption usually appeared early, and was over on the fifth day. The accompanying fever was "putrid or typhous," that is, asthenic. Gangrene was apt to occur on the rump, hips, shoulders, or other parts of the body on which the patient rested, and also on surfaces which had been blistered. Some died so soon as during the eruptive stage; the greater number in the course of the second week. And Dr Gregory, the heedless, indiscriminating shedder of blood, as he has been recklessly assumed to be by the uninformed, adds this emphatic observation—"I found that many of the cases did not bear the usual antiphlogistic remedies"—viz., blood-letting and purging—"which we prescribe in common rubeola, but did better with tonics and the other modes of treating typhoid fever." A very brief allusion to this epidemic, corresponding in terms exactly with that now given, will be found in a review article in the *Edinburgh Medical and Surgical Journal* for July 1809. The reviewer, admitting the general utility of a strenuous antiphlogistic treatment in measles, emphatically declares that it was dangerous in that epidemic, so that even a few leeches occasioned increased difficulty of breathing, and the symptoms of sinking; and he distinctly ascribes the difference to "the peculiar type of the then prevailing epidemic."—(V. 366.)

The disease which prevailed at that time was evidently a rubeola of a type purely asthenic, or adynamic, and intensely so beyond what has ever been witnessed since that time in Edinburgh. From all we can now learn, it appears to have occurred when other fevers, and indeed febrile diseases at large, presented themselves in the opposite or sthenic form, and it attracted great attention on that very account. Nothing can be more interesting, in relation to the pre-

sent inquiry, than the unqualified admission by Dr Gregory, lecturing ten years afterwards, and by the reviewer, writing at the moment, that the favourite remedy of the day for measles and other febrile diseases was inapplicable to that epidemic. The most prejudiced must see in this admission, that the physicians of the day were not the unobservant devotees of either a theoretical principle, or an empirical rule, which they have been charged with being by some of their successors in the present time.

The epidemic of 1807–8 seems to have been, as it were, an interpolation, in the history of the rubeola of the present century. It drew very marked attention when it occurred, as something new to all men. Nothing coming up to it in asthenic malignity has prevailed since in Edinburgh in the epidemic form. Of the causes which produced this interpolation we know literally nothing. But if any one should succeed in discovering the agencies, physical or physiological, connected with any such epidemic that may chance to appear hereafter, I apprehend that great light will be thrown over the mystery which now hangs over the constitutional variations in febrile and inflammatory diseases.

Dr Gregory, in his lectures in mid-winter of 1817–18, mentioned that, for the first time since 1808, he had seen during that very winter a few similar cases, in which antiphlogistic treatment could not be sustained. My own earliest acquaintance with measles, during the summer of the same year, was quite different. Both then, and for many years afterwards, it put on the same sthenic form—evinced by a frequent, strong, incompressible pulse, great heat of skin, and perfect tolerance of blood-letting and other antiphlogistic remedies—which characterised our continued fevers of that period. There are, I feel sure, several members of the medical profession still surviving, who will bear me out in the statement, that such was, at all events, the dominant character of measles till at least the year 1830. For reasons already given, I will not presume to indicate the time at which a change took place. But this much seems certain,—that, for at least fifteen years back, measles has ceased to be attended with the highly sthenic fever which attended it between 1817 and 1830. There has been little indeed of that intensely typhous character which rendered the measles of 1807–8 so peculiar and so mortal. But the pulse has been comparatively soft, or at least easily compressible; the heat of the skin has been decidedly less intense; evacuations by blood-letting have been worse sustained at the moment, and have appeared to retard convalescence; and accordingly the treatment in universal credit, if not positively stimulant, has been far less vigorously antiphlogistic than it was until about twenty years ago.

My impression is, that the change of character, and change in treatment, took place about the same time with the corresponding changes in Essential Fevers. But I have no positive evidence of the fact, such as I have presented in regard to that class of diseases.

Of *Scarlatina* I may venture to speak with greater confidence than of measles, because I have seen a good deal more of it, and from the earliest period of my professional life. I have not, indeed, seen it on anything like the vast scale on which continued fevers have come under my notice. And, unfortunately, I cannot refer to notes taken of it at the time of observation. But I can draw with some confidence upon my memory; and, if my recollections prove conformable with those of other medical practitioners in this city of similar standing in point of time, we shall have a solid body of doctrine, which is not to be upset, or even shaken, by any mere hypothesis of a habit of loose observation, or caprice of practice, on our part in former times.

Every one will acknowledge that scarlatina has shown itself, during the last forty years, with a greater variety, both in its essential and non-essential parts, than any other of the epidemic diseases of Edinburgh. At one period, the affection of the throat has been the dominant part, and the main source of danger; at another, this very peculiar element of scarlatina has been absolutely trivial. In one epidemic, a singular secondary neuralgia, or rheumatism of the joints, was superadded, of which very little has been seen since. At one period we have seen the secondary affection of the kidneys very prevalent; at another, this has been a rare sequel. Sometimes the symptomatic fever has had nothing in it to attract particular attention. Repeatedly, however, it has shown a most deadly asthenic character; so that very early in the eruptive stage, occasionally before there was time for the eruption to be distinctly developed, the circulation has literally sunk, and the subject of this mortal form of scarlatina has perished as if struck down by one of those deadly poisons which slay by causing paralysis of the heart. These are differences which all men of some experience must have observed.

Amidst such incidental variations as these, however, there has been also observable, as the general result, a progressive change in the symptomatic fever from the sthenic to the asthenic type. Between 1817 and 1827, scarlatina, usually by no means formidable, was characterised by a frequent, sharp, incompressible pulse, an intense sensation of heat, an elevated temperature, rising so high as 104° , and occasionally even to 107° ; and, in short, all the phenomena of an inflammatory fever, such as distinguished the two epidemics of synocha in 1817-20 and 1827-28. During the earliest portion of this period of ten years, the cold affusion, the favourite remedy of a still prior period, continued to be prized by many; and I myself often used it, apparently with advantage. But it was soon displaced by general blood-letting. And we bled scarlatina patients again and again to the extent of twenty ounces or more, without fear, without injury at all events, and, I cannot help still thinking, with positive benefit, in lessening the force of reaction, shortening the duration of the fever, and hastening the progress of convalescence.

I will not venture to say at what precise date it was that the fever of scarlatina began to depart from this sthenic type. But it was about 1830, or soon afterwards, that my own attention was specially turned to the frequent occurrence of cases in which sudden prostration took place in the early days of the disease, as indicated by failure of the pulse, nervous exhaustion, and death on the fourth, third, or, in one instance, even on the second day. And it is my persuasion, that these cases happened about the period when an asthenic character began to be impressed generally on the fever of scarlatina, and that blood-letting began to be abandoned in the treatment. At any rate, it is undoubted that, for more than twenty years past, scarlatina has ceased to put on the high sthenic form of the early period,—the pulse being usually soft, or, if jarring, yet easily compressible, the increase of heat moderate, and the prostration of strength out of proportion great, compared either with the force of the fever, or with the duration of the disease. It is, I presume, equally certain, that during that long period no practitioner has ventured, unless in a rare instance now and then, to practise the free evacuations of blood which were formerly the general rule of treatment, or now believes that he could have risked such practice as a general rule without imminent danger. And it is my present conviction—although, on this point, I am willing to be corrected by the testimony of others, based on facts recorded at the time—that the abandonment of blood-letting was preceded by the observation of a change of type in the symptomatic fever of scarlatina.

I do not know whether it has occurred to others to suspect,—but occasionally, during the last three or four years, I have seen a case or two of scarlatina, which led me to hope,—that its old sthenic type may be about to be restored. In one instance particularly, there was so strong a pulse, and such pungency of heat about the fourth day, that I felt I should in the old time have infallibly bled the patient freely, especially as he was a vigorous muscular boy of thirteen. This surmise singularly coincided with the opinion of his parent, a medical man in his youth, but long retired from practice, who urged free blood-letting, as the main remedy in his own professional day. I resisted, however, my own surmise as well as his urgency, and with good reason; for in four-and-twenty hours prostration set in, and, rapidly advancing, proved fatal on the sixth or seventh day. The parent almost upbraided me afterwards for not having given way to his importunity. But this gentleman was in error; and there is not any satisfactory evidence that the symptomatic fever of scarlatina has resumed as yet its former sthenic type in Edinburgh.

III. *The constitutional changes which have taken place in the Acute Inflammations of Edinburgh during the last forty years.*

If it be the case—as I have endeavoured to show, not, I hope,

without some success—that, in idiopathic and eruptive fevers generally, the derangement of circulation which attends them has changed in the course of the last thirty years from the sthenic to the asthenic type, no surprise ought to be felt should it appear that the same change has been also observable in the deranged state of the circulating system which attends the *Acute Local Inflammations*.

This indeed is by no means a necessary sequence. It is true—and it has accordingly been objected—that the first two classes of disease originate, at least for the most part, in specific morbid poisons, miasmatic or contagious, but the inflammations in cold, injuries, and other causes of a general nature; and hence, that we can easily imagine different intensities of the generative poison in the former instances occasioning, in the development of disease, differences for which there is no similar or any other apparent cause in the instance of the acute inflammations.

It is true, likewise—and it has also been objected—that in the case of the inflammations there is a certain well-defined process, or succession of processes, which involve certain determinate textural changes, and which have always followed, and must of necessity always follow, fixed physiological laws: that, therefore, inflammation has ever been, and must have been, the same thing exactly as when it was first described by Hippocrates: that its essence is unsusceptible of any such change as will render so great a variety of treatment necessary, one time with another, as the lavish evacuations of blood practised for at least twenty-five years before 1830, and the almost utter avoidance of that remedy for the last ten or fifteen years.

These two objections may be included in one simple answer. We have nothing to do, in the present question, either with the exciting cause, or with the pathological essence of disease.

In fevers, there is a disturbed state of the circulating system, which generally constitutes one of their leading phenomena, but which is perhaps not absolutely essential to their existence, as at any rate we see occasionally a rare case of fever with a natural pulse. This disturbance I have shown above to be apt to present very variable characters as to type, apart from any discoverable difference in the other phenomena of fever. So, in the acute inflammations, a similar disturbed state of the circulating system commonly prevails; which, however, is certainly not essential, because it is often wanting altogether; but which, when present, as is the common rule, surely becomes an important complication.

It may, in fact, become so in more shapes than one. Sometimes the disturbed state of the circulating system is so asthenic in character, the pulse being so feeble and the nervous energy so prostrate, that the physician, in spite of the presence of local inflammatory action, is obliged to administer stimulants freely, for the purpose of maintaining the circulation. At other times, undoubtedly,

the excited state of the circulation is moderate in all its characters, so that it scarcely affects the result one way or another, and may be disregarded in the treatment. It cannot, therefore, be considered at all wonderful, that the force of the heart and arteries should be on other occasions so vehement, as to exasperate seriously the local inflammation, and to bring about in that way an unfavourable issue. In such circumstances, the reaction of the circulating system must be subdued by reducing measures, not called for under other conditions of the circulation. These are very elementary views. But they have been lost sight of by the opposite side in this controversy; and hence their errors.

It does not appear to me any great assumption to maintain, that one of these three states of the circulation may predominate as the constitutional peculiarity of the local inflammations at one period, and another state at another period. And if it can be shown that the sthenic character of the symptomatic fever had actually prevailed for a sufficient period of time to attract the attention of physicians, and was afterwards supplanted by an opposite or intermediate neutral condition, I apprehend there will be no difficulty in discovering why one mode of treatment was at the same time changed for another. Surely even my colleague, Dr Bennett, will allow, that local acute inflammation may be a very different thing to treat from what he has seen of it, if, along with local inflammatory action, he has to contend with a torrent of circulating fluid driven towards the suffering part with a vehement force,—which we cannot doubt is the case, if the pulse be found persistently full, bounding, hard, and incompressible.

If these introductory views be correct, the only question remaining is—“Whether, during the last forty years, there has been such a change in the character of the symptomatic fever attendant on acute local inflammations, as will account for so great a change in its treatment as the abandonment of blood-letting?” I maintain that such a change has actually taken place in the phenomena of local inflammations as presented in Edinburgh.

Unfortunately, I am not able to refer to any recorded facts in support of this proposition. But I can conscientiously say, that many years have elapsed since I have made allusion to it in my lectures on Clinical Medicine in this University. And I can, with equal confidence, pledge myself to having formed my present opinion long prior to the first germs of the present controversy, and quite irrespectively of controversial bias of any kind.

The local inflammations, in which the change in the form of the concomitant fever has principally attracted my attention, have been erysipelas, acute rheumatism, pleurisy, and pneumonia. In all of these diseases, during the earlier period between 1817 and 1830, or a little later, it was customary to find the pulse not merely frequent but also either full, hard, and difficult to extinguish by pressure, or small, wiry, and equally hard to compress. This state of the pulse

was likewise attended with more heat of the skin and febrile restlessness, than I have been accustomed to observe for many years past. In correspondence with these peculiarities, it was not uncommon to observe the blood from a vein, opened for the purpose of blood-letting, issuing with great force, and of an unusually florid colour, and occasionally with a certain jerk contemporaneous with the pulse. I have repeatedly seen, at the time now referred to, hospital dressers or other young medical men so much alarmed at these appearances, as to fear that they had cut the subjacent artery, and only relieved, upon finding the flow of blood arrested by firm pressure of the vein beyond the wound. In no disease was the force of reaction more remarkably demonstrated by these phenomena than in acute rheumatism. I well remember that in my experiments on the changes produced by the air in the blood, performed in 1830 (See *Edinburgh Medical and Surgical Journal*, xxxv. 94), I was repeatedly unable to use for my purpose venous blood taken from rheumatic patients, on account of its colour being too little removed from that of arterial blood, to show the change of hue caused by agitation with air.

The character of the pulse, which I principally miss in the present time, is its incompressibility, which was a constant object of attention and interest during the fashion of blood-letting, and by which, far more than by the mere frequency or fulness of the pulse, the question of blood-letting and its amount were regulated. Those who have not had occasion to observe this phenomenon in acute local inflammation, will understand what is meant, if they have ever attended carefully to the condition of the pulse in some forms of sudden, violent, brief apoplexy, in which, after a short stage of great depression and irregularity, the pulse gradually rallying, becomes at length almost painfully full and bounding, and so hard to compress that scarcely any force, exercised with the finger's point, will completely extinguish it.

As inflammations with this accompaniment continued to advance, in spite of blood-letting, or when it had been neglected, the condition of the pulse gradually altered, until at length it acquired the same comparatively soft and easily compressible character which is observed to be its ordinary condition at all stages in the present time, as well as for some years past. Now, it is important to remark, that as soon as the pulse put on this altered condition in the advanced stage of inflammation, every medical man practised blood-letting with far greater caution; and I could show, by notes I still possess, of the case of a brother graduate whom I treated at Paris in 1821 in rather remarkable circumstances, that I at least was at that time fully aware of the danger of blood-letting in pneumonia in such a state of the circulation.

If in those days physicians were aware that the acute local inflammations might, in their course, present characters which contra-indicated blood-letting, it does not appear a very violent assumption.

that, if they observed those characters presented at the beginning instead of the advanced stage, they would hesitate to draw blood, and at length, as this character became more and more manifest, abandon it in a great measure; and such I apprehend is the real history of the modern change of men's minds as to the employment of the free evacuation of blood in the acute inflammations.

An attempt has been made to ascribe the change,—1. to an improved acquaintance with the phenomena of pneumonia as one of the leading acute inflammations—the consequence of the use of the stethoscope—and 2. to our acquaintance with the antiphlogistic properties of tartar-emetic. Both arguments are baseless. Acquaintance with the stethoscope will not explain the abandonment of blood-letting in other inflammations besides pneumonia and pleurisy; and yet the remedy was surrendered in all acute inflammations, about the same time. Besides, it is altogether a mistake to assume, that the stethoscope came into familiar use in Edinburgh, and more especially among the medical officers of its Infirmary, only about the time when blood-letting began to fall into desuetude. Several practitioners of this city had studied long before that date under Laennec himself; one of these was physician of the Infirmary so early as 1827; and others besides him used the stethoscope with as much address and familiarity at that period as any of their successors have done since.

As little can it be admitted, that professional men of this city have been acquainted with the merits of tartar-emetic as an antiphlogistic remedy only since it began to displace blood-letting in the treatment of pneumonia. The use of tartar-emetic, as an antiphlogistic in local inflammation in British practice, is of much older date; when I was a young graduate, it was a familiar remedy in the advanced stage of pneumonia, and was used by myself as the acknowledged resource, and with prompt and excellent effect, in the case of pneumonia referred to above in 1821; and as for Laennec's plan by large, frequent, contro-stimulant doses, it was employed in the clinical wards by my predecessor, Dr Duncan, who died in 1832, and likewise by myself and others some years earlier than that date.

I feel persuaded, therefore, that the more this interesting subject is investigated, and the more that the personal experiences of those who practised medicine between 1817 and 1830 is brought to bear upon it, the more will it appear manifest, that a change to a mere asthenic character has gradually taken place since that period in the febrile condition of the circulation attendant alike upon continued fevers, eruptive fevers, and the acute local inflammations; and the more will it appear probable that this change has been the real cause of the change which has taken place in medical opinion and practice as to their treatment.



